INFORMATION AND CHOICE OF A-LEVEL SUBJECTS: A CLUSTER RANDOMIZED CONTROLLED TRIAL WITH LINKED ADMINISTRATIVE DATA

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Abstract

We estimated the effects of an intervention which provided information about graduate wages to 5,593 students in England, using a blinded cluster randomised controlled trial in 50 schools (registration:AEARCTR-0000468). Our primary outcome was students' choice of A-level subjects at age 16. We also recorded the students' expectations of future wages and the A-level subjects they intended to take before and after the intervention, and linked their data into national administrative school examination records. We found that an hour-long lesson on information about graduate wages affected students' beliefs about and choice of subject. They were more likely to take Maths and less likely to take Biology and Computing. We found strong evidence that mediating factors such as their beliefs about average graduate salaries and their own likely salary in each subject were affected by the intervention. This suggests providing accessible and credible information on labour market consequences of school choices may influence students' decisions. In the light of concerns about the quality of careers guidance for school students and expectations that educational choices should be well-informed, the study has clear implications for policy and practice.

Keywords: Subject choice, careers guidance, human capital, salary expectations

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Introduction

We use a cluster randomised controlled trial to test the effect of varying the information given to 15-16 year-old students on their choice of A-level subjects. Students choose 3-4 A-level subjects to study between the ages of 16-18, chiefly as a preparation for undergraduate study. This study bears upon several issues. First, choice of subject matters for subsequent options in higher education with consequences for future earnings (Boliver, 2013; Dolton & Vignoles, 2002). Second, national and supra-national government bodies have claimed that insufficient students are choosing to study STEM subjects (see for example, Browne, 2010; European Commission, 2003). Third, schools in England have been criticised for giving biased and incomplete information about the consequences of subject choice (see for example OfSTED, 2010). Fourth, informing choice has become a popular form of government intervention in education (Authors, 1). This study also contributes to theoretical debate about the extent to which educational choices are firmly framed by social context and personality traits or open to influence through information received.

The first section reviews the role of information in students' subject choice. The second section describes the effects of an intervention on subject choice of providing 15-16 year-old students with two different sets of information. We evaluated the effects of this intervention using a cluster randomised controlled trial.

Our study finds that compared with providing the kind of information typically available to students in English secondary schools, providing information on graduate salaries increases recruitment in mathematics whilst reducing recruitment in biology and computing. This result indicates students' subject choices are potentially malleable and not fully constrained by social background and personality traits.

Information and subject choice

This section starts by examining contrasting predictions about the effect of information on students' subject choices. The second half of the section reviews evidence of students' knowledge and the effect of providing students with additional information.

Why might information about graduate salaries affect subject choice?

Human capital theory has exerted a powerful influence on recent education policy in England (e.g. Browne, 2010) and elsewhere. So this is a natural place to start when examining predicted effects of information on subject choice. From this perspective, the key variable is expected graduate income[i]. At the time of this study, roughly 85% of all advanced level students progressed to higher education (Higher Education Funding Council for England (HEFCE), 2015). Students' choice of (3-4) A-level subjects constrains their choice of undergraduate subject. The 'backwash' effect of aspiration for university subject upon choice of subjects to study has been identified even in Germany where students' choice is more limited than in England (Nagy et al., 2006) and it is emphasised in guidance on subject choice provided by the Russell Group of Universities and UCAS.

The subjects with the highest graduate premium in the UK are: computing, economics and management, engineering, law, mathematics and medicine (Chevalier, 2011; O'Leary & Sloane, 2011; Walker & Zhu, 2011)[ii]. Undergraduate programmes in computing, economics and engineering usually require A-level mathematics. It is, therefore, unsurprising that, as in the US (Rose & Betts, 2004), mathematics is the only school subject which has been shown to be associated with higher earnings in the UK (Dolton & Vignoles, 2002).

Human capital theory also suggests that students should adjust their earnings expectations according to the A-level grades they achieve (Hussain, McNally, & Telhaj, 2009). Students

who achieve higher A-level grades are more likely to achieve a higher class of university degree and more likely to secure a place at a research-intensive university. This means that students must weigh the relative importance of not only the average, but also the variation in graduate earnings across different subjects. If students suffer from 'optimism bias' (Seaward & Kemp, 2000), they will expect above average earnings if they choose a subject in which they have a relative advantage. This could cause reluctance to switch subjects on the basis of difference in mean earnings. Suppose a student is choosing between two subjects where the mean earnings for graduates with subject X are 10% lower than mean earnings for subject Y. They believe that they would earn one standard deviation above the mean for graduates with subject X but only at the mean for graduates with subject Y. Their choice of subject would then be guided by their beliefs about variation of earnings around the meaning. Guidance that students receive to study the subjects in which they get the best grades implicitly encourages them to think that the standard deviation in graduate earnings by subject is large relative to the differences between means. This would provide a heuristic which encourages them to ignore modest differences between earnings by subject (Diamond & Vartiainen, 2007). However, the benefits of choosing 'my best subjects' are conditional on the difficulty of the subjects they have studied (Coe et al., 2008) and whether they have chiefly studied traditional academic A-level subjects (Russell Group, 2010). The Russell Group publicised its A-level preferences one year before this study was conducted and web sites offering advice on choosing A level subjects were already carrying a message about 'hard' and 'soft' subjects[iii].

Alternative perspectives on subject choice – implications for information

This section considers four alternatives[iv]. First, 'Relative Risk Aversion' retains a version of 'rational choice' (similar to Human Capital Theory), but replaces 'utility maximisation' by securing social status that is at least the same as the social status of one's parents (Boudon,

1974). This implies that the critical information is the relationship between a subject studied and subsequent social position. Prospects for social position are perhaps, more strongly associated with the status of university attended (Milburn, 2012) than with variation in degree subjects ordered by graduate earnings. This implies that the critical information for students is the preference of high status universities in England for traditional school subjects (Russell Group 2010).

Second, variation in subject choice by social class and gender has been explained by students' taken for granted sense of what is 'normal' for people like them (e.g. Francis et al., 2016). This portrayal of social and gender identity reproduction generates two views of critical information. Reproduction might be reinforced by information about the distribution by class and gender in (i) who currently studies each subject and (ii) types of employment associated with each subject. Reproduction might be challenged by exemplification of individuals who feel they have successfully broken the mould.

Third, subject choice has been explained (e.g. Elsworth et al., 1999; Hannover & Kessels, 2004) in terms of the match between personal characteristics and the perceived characteristics of a study or occupation. This individual character perspective suggests the critical information required is the capacity of different occupations and associated school subjects to satisfy personal aspirations. Easily accessible online advice[v] usually aligns with this perspective in suggesting that students should choose the subjects they like and the subjects at which they are most successful.

Finally, a fourth perspective (e.g. Warton & Cooney, 1997; Smyth & Hannan, 2006) emphasises the roles of school systems and individual schools in defining subjects and steering students towards subjects which suit the interests and presumptions of teachers,

schools and the governing social class. This perspective suggests that 'normal school practice' in the guidance on subject choice will necessarily act to constrain students' choices in a way that causes a loss of welfare. Students will benefit from independent information about graduate salaries, associations between subjects and social status, exemplification of 'mould breakers' and the match between subjects and character traits.

Several studies in this field (e.g. Van der Werfhorst et al., 2003; Authors, 2; Plinxten et al., 2012) have explicitly combined more than one of these perspectives. We follow this heterogeneous approach in believing that each perspective captures part of the picture. A study of the effect of different kinds of information on subject choice tests a combination of what matters to students *and* what they already know.

What do students already know?

Students' choices are only likely to be affected by information that is new to them.. This section examines evidence of secondary school students' knowledge about the implications of their subject choices. State schools in England have been heavily criticised for the quality of information about subject choice they provide to students (Long & Hubble, 2016; OfSTED, 2001, 2010). Similar problems have been observed in other countries. For example, Warton & Cooney (1997) reported that 20% of the Australian students they surveyed did not know whether the subjects they hoped to study were even available at their current school and half of the students had no idea about whether their preferred subjects were available at

other local schools. Outside of school, students can gather information from online sources. Most sites[vi] (including the Universities and College Admissions Service, UCAS, which is responsible for all undergraduate applications) advise that students should choose subjects they like and subjects 'they are good at'. They also offer advice on the match between A-level choices and undergraduate options. The guidance on the A level subjects preferred by the Russell Group (2010) of research intensive universities features prominently in this advice.

Given this range of sources, what do students actually know? We now briefly review evidence about students' knowledge of (i) their relative strengths, (ii) the subject preferences of Russell Group universities; and (iii) graduate earnings by subject. First, analyses of longitudinal patterns in students' achievements and subject choice (Authors 3; Van De Werfhorst et al., 2003) have shown that students' relative prior performance in different subjects is a more powerful effect on their choice of A-level subjects than either gender, socio-economic status or ethnicity. So, as long as they have studied a subject before, students are reasonably accurate in judging and acting upon their relative strengths. Second, qualitative and quantitative evidence suggests that students attending private schools in England are more likely than state school pupils to be informed about the implications of subject choice for access to study at an elite university (Boliver, 2013; Dunne, King, & Ahrens, 2014). Third, students with lower cultural capital may be less aware of variation by subject in graduate earnings and less likely to choose subjects with a high graduate premium (Authors 4). Fourth, there is evidence of optimism bias in undergraduates' expectations of UK graduate earnings. Students of science, languages and humanities are the most over-optimistic (Jerrim, 2011). School students who have yet to enrol in higher education, are

likely to have less accurate expectations than those reported in studies of undergraduates' wage expectations (e.g. Delaney et al., 2011; Zafar, 2013).

Does information change educational choices?

Unlike many interventions to change students' *understanding*, information interventions to address inaccuracies in financial *knowledge* tend to be short, cheap and effective (see for example, Drexler et al., 2014; Duflo & Saez, 2004; Vignoles & Bhutoria, 2016). For example, Jensen (2010) reported a one lesson intervention in which 14-15 year-old students in the Dominican Republic were given information about graduate earnings. Students who had been in the treatment group subsequently spent between 0.18 years (95% confidence interval: -0.01, 0.37) longer than students in the control group in formal schooling. Kerr et al. (2014) found that school students in Finland who were given information about variation in graduate earnings were less likely to apply for humanities courses in polytechnics and more likely to apply for Social Science or Business and finance in polytechnics. In contrast, McGuigan et al. (2012) found that 14-15-year-old students in London who accessed a web-site providing information about graduate premia and employment improved the accuracy of their knowledge, but with little effect on their intentions to further study. Oreopoulos & Dunn (2013) found that differences between the effects of these trials may be attributable to the form of the intervention (in class or online in students' own time) or the context (e.g. country) in which the intervention took place.

Research questions

In the light of this review we seek to answer two research questions:

(1) To what extent does information change subject choices in secondary schools in England?

(2) To what extent does the information about graduate salaries affect students' beliefs about graduate salaries?

We answer the first question by comparing the effects of information about graduate salaries with the effect of information about the subject preferences of high status universities and the relative difficulty of subjects. The second question bears on the issue of what students do and do not know. The point is not that students should be encouraged to prioritise future earnings. If, for example, information improved the accuracy of students' beliefs about the graduates of subjects requiring mathematics at A level this could help achieve of policy objectives and make it less likely that students made subject choices based on inaccurate beliefs.

Methods

Trial design

We compared the effect of providing two different kinds of information to 15-16 year-old students using a cluster randomised controlled trial (RCT). Our intervention lesson provided information on graduate wages, whilst the alternative lesson provided information on the categorisation of subjects as 'hard' or 'soft'. There are two reasons for choosing this comparison rather than a comparison of one type information with 'normal practice'. First, RCTs can suffer from bias if the allocation to the treatment condition is not concealed from participants because of the placebo effect (Guyatt et al., 2011; Higgins et al., 2011). Participants in the intervention may change their thinking, attitudes and practice simply because they are taking part in the intervention (Hawthorne effect). Participants assigned to a control group will have received some information about the intervention when they were recruited to the trial and this knowledge could invoke changes in thinking, attitudes and practice of teachers. When an intervention is relatively straightforward in intent and content,

the risk of initiating some change in the control group is increased. For this reason, medical trials which are designed to demonstrate efficacy (that a treatment works) compare treatments to a placebo intervention. This deals with potential bias because participants know their allocation to treatment, and whether they are taking the "active" treatment or not.

Second, an intervention in normal school time is always replacing whatever intervention would have happened in 'normal practice'. Therefore, some very strong assumptions are needed to allow the difference between the intervention and 'normal practice' to be interpreted as a generalizable effect. We must assume that normal practice does not activate the causal mechanism which is targeted by the intervention. For example, an intervention designed to test effects of praise on children's work will have a minimal effect size if routine practice already involves teachers in giving praise. It would not tell us whether or not praise makes a difference. If we admit some normal practice might activate the causal mechanism targeted by the intervention, we must assume that normal practice is sufficiently homogenous to avoid a large pooled variance and a tiny effect size even when mean difference is fairly large. These considerations matter if we accept that improving practice rests on understanding processes in education rather than administering doses of specific teaching materials.

Participating schools were randomly allocated to the two arms of the study by a statistician from the University of Birmingham medical trials unit who was independent of the project team. The randomisation was carried out in Stata. The randomization was stratified by three variables: state or private school, single or mixed sex school, average pupil achievement above or below the median for the whole sample to create 8 blocks. Participating schools were blinded to the allocation between the two arms of the trial. The total project sample

included 50 schools and 5,593 students. We report results using an "intention to treat" analysis, as recommended by reporting guidelines for randomised trials (Moher et al., 2001). Intention to treat analyses compare the outcomes of individuals allocated to treatment to those allocated control. This is as opposed to a per-protocol, or as-treated analysis which uses the treatment they actually received. Hence we report differences in outcomes on the basis of allocation to treatment and control rather than per protocol.

The primary outcome of the trial was actual choice of A-level subject. These data were collected from schools in late October and early November of the academic year following the intervention. We used a questionnaire to gather data on students' characteristics and motivations towards choice of subject. The secondary outcomes were expectations of graduate salaries and change in intentions to study different A-level subjects before and after the intervention. We registered the trial with the social science registry (AEARCTR-0000468) and the trial protocol is available on the project web-site. The project received ethical approval from the University of Birmingham ethics committee (ERN_10-1340). Participating Schools and students provided informed consent.

Intervention

We designed two one hour lessons (A) and (B). Lesson A provided information about graduate earnings by subject. Lesson B provided information about subject difficulty and the subject preferences of the Russell Group universities. Descriptions of the lessons and teachers' notes are available in online appendices. The existing literature suggests that students were likely to have less information about earnings than about 'hard' and 'soft' subjects. Each lesson provided incentives for students who were unfamiliar with the information to change their subject choice:

Lesson A (Online Appendix 1): an incentive to choose subjects with higher graduate premia;

Lesson B (Online Appendix2): an incentive for students expecting A-level grades typically required for entry to Russell Group universities to choose traditional, academic, A-level subjects (English, Mathematics, Science, Modern Foreign Languages[vii], Geography, History) and an incentive for students not expecting high grades to avoid 'hard' subjects (such as Modern Foreign Languages, Physics, Chemistry) and to choose 'easier' subjects (such as Art, Business Studies, English and Sociology).

Lesson A presented information on graduate premia for ten subjects: Business, Education, Engineering, History, Languages, Law, Maths or Computing, Politics, Psychology, and Science. The data were drawn from O'Leary & Sloane (2011) and the relative average salaries are presented in **Table 1**. The data were used in three activities within a lesson lasting roughly one hour[viii]. The activities compared absolute average wages for males and females. Other studies, such as Walker & Zhu (2011) have found slightly different relative wages for subjects. But the overall message about graduates' wages from different studies is broadly the same for each study. Maths, Engineering and Computing graduates earn, on average, substantially more than graduates in pure science. Languages graduates' earnings are below pure science, whilst humanities graduates earn the least.

Table 1 about here

The sources of information used in Lesson B were: (i) a web-site on subject choice[ix]; (ii) The Russell Group (2010) and (iii) Coe et al. (2008) who reported the relative difficulty of different A-level subjects. None of these publications or the lesson activities contained any information about relative wages of graduates from different subjects.

Sampling procedure

Based on our previous study (Authors 3) we estimated that we would have 80% power to detect a 11 percentage point difference in outcomes using a two-tailed test at $\alpha = 0.05$, assuming an intraclass correlation of 0.1 and an average of 83 A-level students per year and a sample of 48 schools. We expected to substantially increase our power by adjusting for baseline covariates which associate with student choice, particularly their stated preferences for subjects prior to receiving the intervention lesson. Furthermore, our power was increased because on average the schools in our study had more students than we expected in our original design. Figure 1 presents a flowchart of the sampling process, randomisation and attrition.

Figure 1 about here

We generated a list of all schools within a large and diverse geographical area in England which satisfied our criteria. (details provided in Appendix 1). These criteria were used to reduce attrition due to being unable to trace students who had not continued at the same school and to focus our sample on students achieving at least minimum qualifications for university entry. We divided these schools into two lists (state and private schools) and randomised each list. We approached schools in this random order until we had recruited 20 private schools and 30 state schools. This stratification was used (in the light of the literature on school type and subject choice) to maximise our power to detect heterogeneity in the effect of the intervention across school types. This sampling strategy means that the average achievement of students in our sample was higher than average for all 15-16-year-old students in England. The majority of the students in our sample achieved grades at age 16 which are regarded as a minimum for entry into higher education. Only 9% and 10% of our sample failed to achieve at least GCSE grade C in Maths and English respectively. Eleven

percent of our sample were ‘marginal students’ in terms of GCSE grades (Authors 5 in that their English and Maths GCSE grades summed to 8 points (equivalent to grade C in both subjects). Further details about our sample are available in Authors 4.

Sample characteristics

We gathered data on participating students through questionnaires before and after the intervention. A baseline survey was administered in the first half-term (September-October, 2012). The interventions took place in the final five weeks of that term. The follow-up survey was completed shortly after the intervention before schools had asked students to indicate their provisional subject choices for the following year. The questionnaires asked students to state their intentions towards studying each of several subjects: Art, Biology, Business Studies, Chemistry, Computing, Design and Technology, Economics, English, Geography, History, Languages, Maths, Media Studies, Music, Physical Education, Physics, Psychology, and Travel and Tourism. Students were also asked to indicate what they believed graduates would earn at age 30 if they studied any of: Art, Business Studies, Education, Engineering, History, Languages, Law, Politics or Sociology, Maths or Computing, Physics or Chemistry or Medicine. For each subject area, students were asked to express their expectations of (i) average graduate earnings; (ii) the earnings of a graduate just in the top quarter of earners for that subject; and (iii) the earnings of a graduate just in the bottom quarter of earners for that subject. They were also asked to indicate what they thought they would earn if they studied that subject at degree level. Our data enable us to investigate intervention effects on the probability distribution of students’ expectations (Manski, 2004) as well as their expectations of average graduate earnings. As far as we are aware this is the first study to provide evidence of students’ expectations of average graduate earnings *and* their expectations of their own earnings before they attend university. This enables our analysis to take account of rigidity in

personal wage expectations when beliefs about average relative salaries change. We also asked students to indicate the relative strength of different motivations (including future salary) in their choice of subject to study[x]. These data were matched with pupil level information from the National Pupil Database (NPD). Descriptive statistics are presented in Table 2. There were no significant differences between the characteristics of students assigned to each lesson.

Table 2 about here

Imputation of missing data

Ten schools withdrew part way through the project. Six schools (571 students) allocated to treatment arm did not take part in the intervention, and four schools (487 students) allocated to control did not participate in the second round of questions. Six schools cited practical problems: staff illness or workload or an impending school inspection. Four schools stated that they were uncomfortable with the data in the lessons on the grounds that the information might influence students' choices in ways that was not acceptable to the teachers in these schools. Two of these schools were in the intervention arm and two of the schools were in the control arm of the trial[xi]. There were missing values for some participants due to student absence for the lesson or for one of the questionnaires and students not giving permission for their answers to be used.

We imputed missing values using the multiple imputation by chained equations using (`ice`) in Stata (Royston, 2009). We used the following variables in multiple imputation: wage expectations, family background, expected exam results, Key Stage 2 and 3 exam results, whether the students intended to study each subject at A-level and the students' actual A-

level choices, and the stratification variables: private or state school, above or below median achievement, and single sex school. We imputed 20 imputation datasets with 20 cycles. We have a rich set of background and post-intervention data, including data on intentions, socioeconomic status, and academic attainment precisely measured from linked administrative data. Multiple-imputation allows for missingness under the missing at random assumption. This assumes that for any variable, conditional on the observed data, the missing values have a similar distribution to the observed values. We also report a complete case analysis which depends on the stronger assumption that individuals with any missing values are a random sample of the experimental sample. Therefore, as per established guidelines (Wood, White, & Thompson, 2004) for reporting randomised trials, we report the complete case analysis restricted to individuals with no missing data as a sensitivity analysis.

Statistical analysis

We report the balance of characteristics of the students between the two arms of the trial, and test for the intention to treat effects of the intervention on the students' choices and beliefs using logistic or linear regressions. This treats each student's beliefs about and choice of subject as independent. We relax this assumption below in a sensitivity analysis using a bivariate probit regression. The confidence intervals and hypotheses tests for all analyses allow for clustering of students between schools.

We estimated the effect of the intervention on the odds of taking each subject, using the odds-ratio scale, adjusted for the stratification variables (Kahan, Jairath, Doré, & Morris, 2014). We also report a fully adjusted analysis where the results are adjusted for a range of baseline characteristics, gender, achievement at age 14 in English, Maths, and Science, the students'

expected grades in English and Maths at age 16 (GCSE) and the students' intentions towards studying the subject prior to the intervention as shown in equation 1.

$$logit\ (p_k) = \alpha_{k0} + \alpha_{k1}x_1 + \alpha_{k2}x_2 \qquad (1)$$

where the subject choice for each of *k* subjects is indicated by the binary variable p_k, equal to one if the student took the subject. The intervention arm is indicated by x_1, equal to one if the student attended a school allocated to the intervention. The other covariates, such as prior intentions on taking a subject, are indicated by the vector x_2. Therefore, the parameter of interest is $\exp(\alpha_1)$. This parameter has a simple interpretation as the ratio of odds of taking the subject in the intervention and control arms. The adjusted analysis absorbs some of the heterogeneity in the outcome, so is likely to be more precise.

We estimated the effects of the intervention on the students' expectations of graduates' wages and their own wages across a range of subjects using linear regression, shown in equation 2.

$$w_k = \beta_{k0} + \beta_{k1}x_1 + \beta_{k2}x_2\ . \qquad (2)$$

For these results, the parameter of interest is the mean difference in wage expectations, w_k for degree subject *k*, between the intervention and control arms, indicated by β_{k1}. As with the results above, we also report basic and fully adjusted results adjusted for the participants' prior wage expectations.

For subjects in which we measured both the students' intentions to study the subject and their wages expectations (Business, History, Languages, Maths, and Physics) we investigated whether there were any differences in the effects of the intervention in students who initially stated they were likely or definitely going to take a subject.

We investigated whether there was any effect of the intervention on students' stated intentions of taking each subject measured on a Likert scale using linear regression. The parameters of interest are the mean difference of student intentions to take each subject between the intervention and control arms. Again to increase the precision of our results, we also report these results using a basic set of covariates and full set of covariates including the students' initial intentions of taking each subject. Please see the online appendix for further sensitivity analysis.

Sensitivity analyses

We carried out sensitivity analyses via a complete case analysis and our modelling of students' choice. In Authors (3) we estimated a multivariate choice model with multiple outcomes which allowed for correlations between the choices. However, we were not able to jointly estimate these models using data for all the subjects reported in this paper. We attempted to fit a multivariate choice model for all subjects, but it did not converge. In a simplified analysis, we investigated whether allowing for bivariate correlations between subjects meaningfully affected the results. We used the imputed data and for every possible pair of subjects, we estimated a bivariate probit model, which allows for correlations between the students' choices of two subjects. We included the same covariates as described in our primary analysis above. Our results were substantively unchanged, we report the p-values on the effect of the intervention in the appendix. We investigated whether our adjusted results were sensitive to adjusting for Key Stage 2 English, Maths and Science results at age 11, rather than Key Stage 3 at age 14 as a sensitivity analysis. This made little difference to our results. In an exploratory analysis, we investigated whether there was any evidence for heterogeneity in the treatment effect by gender and between private and state schools. We

report standard errors clustered by school for all statistical tests and a sensitivity analyses restricted to individuals with complete data in the online appendix.

The appendix contains a sensitivity analysis of the results reported in the main paper, but restricting the sample to participants with no missing data. In the paper, we refer to this as the "complete case analysis". All other details of the analysis remain identical to those described in the paper, standard errors are clustered by school. Sample sizes are reported in the tables. The code used to produce the results reported in this study can be accessed here (details removed for review).

Results

Of the initial 5,593 students eligible to take part in the study, 4,539 took part in the initial survey, 4,435 took part in the second survey and 46 schools provided information on the actual choices of 3,594 students. Thus in our multiple imputed results, we have 5,593 students, 3,334 allocated to the intervention and 2,259 allocated to the control lesson.

Table 3 and Figure 2 here

The effects of the intervention on the likelihood of taking each subject are shown in **Table 3** and **Figure 2**. Of those students who remained in the same school, students in Lesson A receiving information on graduate salaries were more likely to take Maths (52% vs. 42%) and Chemistry (33% vs. 25%) than those in Lesson B. Students in Lesson A were 48% (95% confidence interval (95%CI): 9% to 100%) more likely to take Maths, 43% (95%CI: 3% to 98%) more likely to choose Chemistry, and 36% (95%CI: 2% to 81%) more likely to choose Physics, but were 35% (95%CI: -4% to 59%) less likely to choose computing. At first sight the effect of the intervention in reducing enrolment in Computing looks odd given that

students were given information about graduate salaries in 'Mathematics and Computing'. However, there were negative correlations between actually studying Computing and actually studying either of Mathematics, Physics or Chemistry. We comment later on the implications of these correlations in the light of changes in students' graduate wage expectations. There were differences for the other subjects although these were too imprecise. Adjustment for demographics, prior exam results, and prior intentions increased the precision of the results. In the fully adjusted results (right-hand column) the effects of the intervention on choice of Maths, Chemistry, Physics were attenuated, but the effect on Computing increased. The results for other subjects were imprecise and consistent with relatively large increases and decreases in enrolment.

Tables 4, 5, and 6 here

We investigated if the effects of the intervention were mediated by students' beliefs about graduate wages (**Table 4**[xii]). Students initially believed that Law and Medical graduates had the highest average salaries. They also believed their own earnings would be highest in these subjects. We tested whether the intervention affected students' beliefs using linear regression (**Table 5**). The intervention caused students' beliefs about average salaries of graduates in Politics or Sociology to fall by £2,956 (95%CI: £2122, £3790) and Law graduates to fall by £2,823 (95%CI: £1,967, £3,680). Students' reduced their expectations of History, Languages, Medicine and Physics or Chemistry graduates, whilst their beliefs of education graduates' salaries increased. Although there was little evidence the intervention had substantive effects on their beliefs about average Maths or Computing graduates' salaries, the large reductions in students' beliefs about the salaries of graduates in most other subjects suggested that Lesson A raised expectation of salaries of Maths graduates relative to others. Moreover, expectations

of engineering graduates' wages rose substantially. This change in expectation was negatively associated with studying Computing and positively associated with studying either of Maths or Physics. There was weaker evidence that the intervention affected students' beliefs about their own future salaries in each subject. The students' expectations of their own salaries if they took education and engineering degrees substantially increased, their expectations of Law, Medicine, and Politics fell. We found little evidence that the effects of the intervention on the students' actual choice of subject was mediated by its effects on their salary expectations (**Table 6**).

Table 7 here

Finally, we investigated whether the intervention had any effects on the students' stated preferences using multinomial logistic regression (**Table 7**). After adjustment for prior intentions on taking each subject, fewer students intended to take Biology, Chemistry, English, Geography, and Languages. There was little evidence of an effect on their in intentions to take Business, History, Maths, Physics and Psychology. More students stated they intended to study Computing. When we restricted the analysis to students who actually studied Maths we found that whilst the control students did not change their intentions to study Maths, there was a 16 percentage point increase in the proportion of students in the intervention group declaring that they would probably or definitely study Maths. In a sensitivity analysis allowing for bivariate correlations between subjects, we found few differences with the main results. This suggests that correlated choices are unlikely to affect our results (**eTable 7**). The results were substantially unchanged when we adjusted for Key Stage 2 results in English, Maths, and Science (**eTable 8**). The effects of the intervention were similar in state and private schools (**eTable 9**). The intraclass correlations for subject

choices across schools can be seen in **eTable 10**. There was little difference in probability of missing outcome data between the arms of the trial (risk difference=-2.2% 95%CI:-13.0%, 8.7%).

Discussion and Conclusion

We found that an hour-long lesson on information about graduate wages affected students' beliefs about and choice of subject. They were more likely to take Maths and less likely to take Biology and Computing. We found strong evidence that mediating factors such as their beliefs about average graduate salaries and their own likely salary in each subject were affected by the intervention.

Higher education policy in England increasingly relies on market forces to allocate students to courses and resources to universities. However, undergraduate recruitment to STEM courses has remained an area of government concern and intervention. Earlier policy, such as Roberts (2002) asserted that university applicants were not responding adequately to high salaries being offered in the labour market for science graduates. This assertion is consistent with evidence of salaries for UK engineering graduates but it is not consistent with evidence of salaries for graduates in pure science. More recent policy, such as Browne (2010) has argued that applications to science courses in higher education will fall below the socially desirable equilibrium because students will not take account of positive externalities. The implementation of the reforms to higher education funding proposed in the Brown Review has seen universities accepting subsidies for science courses whilst charging identical tuition fees to STEM and non-STEM students. The policy has resulted in little additional incentive to study STEM subjects.

The impact of government interventions in higher education depend on students' knowledge as well as on the choices of providers. Jerrim (2011) suggested that applicants to undergraduate courses in the UK tend to overestimate their own future earnings. More specifically, they tend to believe that the graduate premium for pure science is the same as the graduate premium for Maths and applied science. Our results support this conclusion.

The key addition from our study concerns the effect of providing school students with information about graduate salaries. Human capital theory predicts that students will change their choices if they are better informed about monetary benefits of different options for study. Students' beliefs about graduate premia for Maths and Computing were confirmed by the information in the intervention. However, students' beliefs about graduate premia for Pure Science, Sociology, Politics and Law were challenged. A significant number of students reduced their wage expectations and there was a small but significant reduction in intentions to study these subjects in their final years of schooling.

Generalizability

Our sample comprises a high proportion of schools with high achieving students, however, because we randomly selected schools that met the inclusion criteria they should be representative of this type of school. Furthermore, because we evaluated the intervention using a randomised design the estimates of the causal effect of the intervention are likely to be internally valid. Nevertheless, our results raise further questions, such as whether similar interventions can be used to affect educational choices of other groups of students, for example, disadvantaged students eligible for free school meals, or under-represented groups, such as women in engineering. Further randomised trials are needed to determine how generalizable these results are to all schools and possible educational choices.

Strengths and limitations of the study

Each student had a range of possible subject to choose. This means that our results may be affected by multiple hypothesis testing. However, we found evidence of effects on three of twelve subjects at $p<0.05$. This suggests these results are unlikely to be due to chance. Furthermore, we found further evidence that the intervention affected mediating beliefs about wages and intentions to study each subject.

As with many randomised controlled trials, our study suffered from missing data. This could introduce bias into the results if it is not properly accounted for. We addressed this issue using multiple imputation. This approach will work well in our study because we have outcome data (the actual A-level choices) for a large proportion of our sample. We have the students' actual A-level choices for most of the schools regardless of whether they took part in the second wave of surveys. Multiple imputation depends on the assumption that the data are missing at random, and our rich set of background characteristics, including exam results from five years before the intervention took place, means that the assumption is plausible. Additionally, a complete case analysis (reported in the appendix as a robustness check) leaves our results substantively unchanged.

This is the first randomised controlled trial in the UK to demonstrate that students' educational choices can be affected by providing information on returns to schooling in a structured lesson. To increase the certainty of the effects of this intervention this experiment should be replicated (Ioannidis, 2014; Open Science Collaboration, 2015).

A strength of our study is that we measured students' intentions for studying each subject and their expectations of wages prior to the intervention and then measured their intentions and

expectations after they received the intervention or control lessons. This has two benefits, first, we can tell if the intervention has an effect on these intermediate outcomes, second we can use the baseline intentions to absorb the baseline heterogeneity in choices. This increased the power of our experiment to detect a difference in choices between the two arms of the study.

Policy implications

One possible policy response is to provide students with labour market information. There is some variation in estimates of the size of differences in graduate premia, but the message is the same across all of the studies of UK data: premia for Maths and applied science are high but premia for pure science are modest and premia for humanities relatively low. This study suggests that an information strategy focusing on Maths could change the pattern of subject choice. Providing information to schools is a cheap and possibly powerful policy intervention.

Future research

Future RCTs could examine students' choices at alternative margins of educational choice. For example, do students who choose to leave school at 17 have different beliefs about the returns to education than those who stay on? Is it possible to affect these beliefs by providing credible information? Similarly, do disengaged students have different beliefs about the financial returns to education? The students involved in this study will have chosen their university courses, so one avenue for future study is to examine whether our intervention affected the students' university choices. The intervention encouraged more students to take Maths, therefore it would be interesting to find out if there are differences in the average

Maths A-level results between the arms of the trial. We might expect the results to be lower in the intervention arm because less able students took Maths. Finally, in the future, it may be possible to link earnings data to the participants of the trial to see if the intervention affected the students' earnings in the labour market.

Summary

We conducted a cluster randomised trial of providing information to school students about graduate salaries. We found that the intervention encouraged more students to take Maths. Policymakers are frequently concerned with encouraging sufficient students to take STEM subjects. The results of our trial suggest simply telling students what is in it for them could be sufficient to affect their choices.

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Notes

[i] Unlike in countries like Australia, tuition fees do not vary by subject. Differences by subject in the cost of providing tuition and government subsidies are not reflected in fees paid by students.

[ii] Studies using different data sets come to the same conclusion. Chevalier (2011) used data from the 'Longitudinal Destinations of Leavers of Higher Education which provide information on graduates three years after graduation. O'Leary & Sloane (2011) and Walker & Zhu (2011) used a nationally representative dataset of the whole Labour Force. Therefore, if there had been a substantial change in relative graduate earnings by subject, these two studies would misrepresent prospects for new graduates. However, O'Leary & Sloane (2011) show that there was no substantive change in relative graduate earnings by subject between the mid-1990s and mid-2000s.

[iii] For example, the web site Studential asserted that Russell Group universities had blacklisted soft subjects like Dance, Sports Studies, Photography and Accounting. This view was also reported in national newspapers: https://www.theguardian.com/education/2010/aug/20/a-level-subjects-blacklist-claim

[iv] Although these perspectives are conceptually distinct they are not mutually exclusive. Indeed, Van der Werfhorst et al. (2003) explicitly combine perspectives 1 and 2 and Plinxten et al. (2012) explicitly combine perspectives 2 and 3.

[v] See for example https://www.myworldofwork.co.uk/my-career-options/choosing-my-subjects.

[vi] We base this judgement on a review of the first 20 sites in a Google search using "Choosing A levels". These included the Universities and College Admission Service (UCAS) and the 'Which' organisation. Beyond the first 20 the majority of sites were individual schools, colleges or universities.

[vii] The most frequently studied are French, German and Spanish.

[viii] The project activities and baseline questionnaire are available at http://www.birmingham.ac.uk/research/activity/education/projects/subject-choice.aspx

[ix] 'Studential" http://www.studential.com/further_education/alevels/choosingyouralevels

[x] Reflecting the arguments of Expectancy Value Theory as well as the economics of non-pecuniary incentives.

[xi] Lesson materials were sent to the schools after they were randomised to the treatment or control arms of the project. If schools had seen both lesson materials before the allocation then we would not have been able to rule out leakage from one arm of the trial to the other. As the number of schools withdrawing due to unhappiness with the materials was the same for each arm this should not introduce bias into our results.

[xii] We do not report expectations for Art graduates as the sample size for those actually choosing Art is too small to make sensible inferences.